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An attempt to correct some  
of the misstatements made  
by Sir Victor Horsley ...



DEPARTMENT OF APPLIED MATHEMATICS  
UNIVERSITY COLLEGE, LONDON

*Questions of the Day and of the Fray*

No. III

An attempt to correct some of the mis-statements made by Sir Victor Horsley, F.R.S., F.R.C.S., and Mary D. Sturge, M.D., in their Criticisms of the Galton Laboratory Memoir: 'A First Study of the Influence of Parental Alcoholism, &c.'

BY

KARL PEARSON, F.R.S.

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*Dove si grida, non è vera scientia.*—LEONARDO DA VINCI.

IT is not possible to correct the whole of the mis-statements and misunderstandings with regard to the Galton Laboratory memoir exhibited in the recent papers by Sir Victor Horsley and Dr. Sturge; my time is much better occupied. It must suffice here—and even this will take considerable space—to illustrate the nature of these authors' criticisms and the character of the phrases and assertions which they confidently attribute to the Galton Laboratory staff. In order to do this effectively it is needful to place before the reader again the exact scope of the Galton Laboratory memoir, and indicate the perversions of that scope and of our very words that Sir Victor and Dr. Sturge have not once, but many times repeated. The scope of that memoir was distinctly stated in the paper itself, namely to ascertain within the limits of our data, whether the alcoholism of the parents had a *marked* influence on the mentality and physique of the offspring as children. Now there are two points here to be considered: (i) the definition used by us of alcoholism, and (ii) the limitation of the inquiry to the effect on the offspring as children. Sir Victor Horsley and Dr. Sturge tells us that the correct title of our memoir ought to have been 'Children of School Age'. We did

not write for people who only read the wrappers of papers. On the very first page of our memoir occur the following words :

‘The child may be physically and mentally fit, and yet when adult may exhibit alcoholic tendencies. This is the direct heredity of alcoholism. It is a subject not touched on in this paper. . . . It may be demonstrable to the hilt, and possibly justify the seclusion of the alcoholic ; it does not occupy us in this present study ; we are concerned only with the offspring of the alcoholic as *children*’<sup>1</sup> (p. 1).

In the ‘conclusions’ the words ‘child’ and ‘children’ are used repeatedly, and facing the conclusions are tables giving the ages of the children considered ; references to the ages of the children occur frequently in the text, and on p. 6 the average ages of the sons of drinking and non-drinking parents are actually stated as 9·8 and 9·4 years respectively. Sir Victor Horsley and Dr. Sturge tell us under a heading *Unscientific Use of Terms*, that a second instance of this is our misuse of the term ‘offspring’. They further go on to say that we ought at least to trace the effect of alcohol beyond the age of 14, and that Dr. Maurice Craig has pointed out that the next two decades following the fourteenth year are those during which symptoms of degeneracy usually appear.<sup>2</sup> Why we *ought* to have done what we initially excluded from the exact ‘universe of discussion’ is not obvious ; and the only misuse of terms that we can discover is the attempt of our critics to foist into a word, the use of which is expressly defined on our first page, a meaning which we had excluded from it.

The next point is our definition of ‘sober’ and ‘alcoholic’.

<sup>1</sup> *Italics* in the original.

<sup>2</sup> If Dr. Craig’s view be correct, why have Sir Victor and Dr. Sturge quoted with approval Dr. Nichol’s observations on *school children of like age to ours*? See their *Alcohol and the Human Body*, 1907, p. 325.



The meanings which we gave to these words are stated on p. 3 of our paper:

‘By the term alcoholism in this paper is not necessarily meant the “chronic alcoholism” of medical literature. We believe that many, possibly the majority, of our drinking class would be found to suffer more or less from chronic alcoholism; they at any rate in the opinion of trained social workers—assisted by the judgement of police and employers—are drinking more than is good for them or their homes. On the other hand by “sober” is not meant total abstinence, but cases in which the use of alcohol is so moderate, if it exists, that it does not appear to interfere with the health<sup>1</sup> of the individual or the welfare of the home.’

Now it is perfectly open to Dr. Sturge and Sir Victor Horsley to say that they do not agree with this definition of drinking and sober, but they have no right to say that we are misusing a term, the use of which we have at the outset defined. And we venture to think that most practical men will be willing to accept such definitions as we have given. The evil of alcohol drinking is either graded or it is not. If you assume it to be graded, then there is no doubt into which of our two classes the bulk of those parents must fall whose alcoholism is asserted by Sir Victor Horsley to affect markedly the physique and intelligence of the children, and the effect ought to be visible in the statistics. If you do not accept the graded effect of alcohol then you are driven to the view that whether one glass of beer a day be drunk, or the parent be daily in a state of drunkenness, the influence on the offspring remains the same. In the

<sup>1</sup> Sir Victor Horsley and Dr. Sturge confidently assert (*B. M. J.*, January 4, 1911, p. 73) that we know nothing about whether the health was or was not interfered with by the alcohol, yet the authors of the *Report* directly tell us that they have put under the heading of ‘drunken’ families all those who in a less or greater degree suffered in *health*, diet, and morals from the presence of drinking habits.

Edinburgh Report in every case where drinking was found in connexion with either parent it was stated, and this statement depended upon a very ample inquiry from police, employers, schoolmasters, missions, district visitors, &c., who would certainly know whether drink was interfering with the welfare of the individual or of the home. Now Sir Victor Horsley and Dr. Sturge say that this is the first and worst instance of our use of scientific terms. They further hint that we have gravely deceived the public into the belief that our *Memoir* is a scientific presentation of the subject. I submit that the only grave deception of the public that can arise is to assert that we have used words and reached conclusions of a wholly different sense from those we have ourselves adopted and clearly stated. Illustrations of such assertions I will now provide. On p. 72 of their criticisms they assert that in our opinion :

‘Alcoholism causes no appreciable detriment to the drunkard or his children.’

I think we may fairly ask who is deceiving the public when such a statement is thrust into our mouths. No such statement has ever been made by either Miss Elderton or myself. We object to the manner in which Sir Victor Horsley and Mr. Keynes use the words ‘drunkard’ and ‘drunken’, because they do not fully represent the alcohol using class as defined by us. Any one reading the above statement of Sir Victor Horsley and Dr. Sturge would suppose that we had asserted that the home environment of drinking parents was no detriment to the children. Yet what again did we actually write :

‘Alcohol may thirdly be the source of evil to the children, not because of physical changes wrought in the parents, but because of economic and moral changes produced in the home environment. Mental and moral

degradation of the parents, distress and poverty in the home may, and probably do, follow in the train of intemperance. Money spent excessively on drink means less money spent on the necessities of life; it leads to neglect of the children, to unhappy homes, and to undesirable environment. In any consideration of the results of alcoholism these very obvious facts arrest our attention, and we are inclined to lose sight of the really fundamental question: What is the quantitative measure of these environmental influences on the physical and mental characters of the offspring? (*Memoir*, p. 2).

Again :<sup>1</sup>

‘These results are certainly startling and rather upset one’s preconceived ideas, but it is, perhaps, a consolation that to the obvious and visible miseries of the children arising from drink, lowered intelligence and physique are not added.’ (Ethel M. Elderton. *The Relative Strength of Nurture and Nature*. Lecture, Series III, p. 26.)

The question before us was not whether there was greater misery to the child, but whether there was either a toxic or an environmental influence of the alcohol of the parent upon the mentality or physique of the offspring. Now first we actually did find a greater death-rate among the children of the alcoholic parents. If our two groups represented simply a ‘confusion of statistics’ as Sir Victor Horsley and Dr. Sturge assert, why should this greater death-rate be quite sensible when we use throughout exactly the same process of differentiating the alcoholic and non-alcoholic groups? The death-rate of children of sober parents is from 25 to 28 per cent.; that of children of drinking parents is from 33 to 36 per cent., giving a ratio of from  $\frac{3}{4}$  to  $\frac{7}{9}$ .

<sup>1</sup> With this passage undoubtedly before them, for they quote the word ‘startling’ from it (*B. M. J.*, 1911, p. 77), Sir Victor and Dr. Sturge do not hesitate to state that we have asserted that alcoholism ‘causes no appreciable detriment to the drunkard or to his children’. How is it possible to deal with critics who at every fourth or fifth line of their paper deliberately put into our mouths statements we have not made?

This applies to both the Manchester and Edinburgh data alike. Roughly this ratio may be taken as  $4/5$ , or an excess of  $1/5$  in the deaths of children of drinking father or mother has to be accounted for. Now how did we write about this excess? Quite undogmatically and as follows (p. 27):

‘Some of this excess of child deaths is certainly due to accident, to overlaying, to burns, and to other causes arising from carelessness, but we should be inclined to attribute it, at least in part, to the same causes, probably to want of home care, to food defects, perhaps to other factors possibly toxic, which show themselves in slightly less height and weight among the children of the drinking mothers when these children reach a school age.’

When we remember that there is a fairly high correlation between the use of a ‘dummy teat’ or ‘baby pacifier’ and infantile mortality, the reader will understand what we mean by the carelessness or uncleanness that leads to the death of a child.

Our final conclusion on this subject was (p. 31):

‘There is a higher death-rate among the offspring of alcoholic than among the offspring of sober parents. This appears to be more marked in the case of the mother than the father, and since it is sensibly higher in the case of the mother who has drinking bouts than of the mother who habitually drinks, it would appear to be due very considerably to accidents and gross carelessness, and possibly in a minor degree to toxic effect on the offspring.’

Why did we not attribute it to toxic effect, or only to that source in a minor degree? Because our statistics showed no such differentiation in general health between the children of the two classes, and because the effect increased with the ‘bouting’. Notwithstanding that we *directly* appealed to carelessness and neglect in the home environment as a source of the higher death-rate, what statement do Sir Victor and Dr. Sturge put into our

mouths? Brazenly they write that we assert that 'alcoholism causes no appreciable detriment to the drunkard or his children' (*B. M. J.*, p. 72).

Sir Victor Horsley, in his speech at the Temperance Medical Breakfast in July, 1910, used the following words :

'In spite of their general conclusion that probably the children of alcoholic parentage were just as well as the children of the moderate drinkers, they notwithstanding came to the conclusion that these equally healthy children died at a much earlier age than those of the abstainers. Now I cannot understand how one particularly healthy child can expire sooner than another' (*N. T. Q.*, p. 143-4).

We endeavoured in the first edition of our first *Memoir* to enlighten such ignorance by using the word *surviving*. But we still appear to have failed to reach Sir Victor's understanding. Let us suppose, merely as illustration, that the children of both the sober and drinking groups to have initially equal average health. The environment of the offspring of drinking parents will be harder. More children will be, and actually are killed off, and these children will, on the whole, be the weakest third of the child population. A lesser destruction, also of the weaker element, takes place among the sober; accordingly among the *survivors* of school age it would be quite possible for the children of the drinkers to show a higher standard of health than the children of the sober. It is a question of whether selection or hard environment produces the greater influence on the health. We worded our conclusion on this point as follows:

'The source of this relation [fewer delicate children among those of alcoholic parentage] may be sought in two directions; the physically strongest in the community have probably the greatest capacity and taste for alcohol. Further, the higher death-rate of the children of alcoholic parents probably leaves the fitter to survive' (p. 31).

To assert, as Sir Victor and Dr. Sturge do, that 'children



who are healthier ought not to die more' is pure dogma until we have considered whether the two sets of children have been submitted to the same degree of stress in the environment. We actually used the greater degree of stress in the environment to account for the higher death-rate accompanied by equal, if not greater health among the surviving children of the alcoholic.

Now Sir Victor Horsley and Miss Sturge (*B.M.J.*, p. 76) write as follows :

' In their *reprinted*<sup>1</sup> memoir they take up this [V. H.'s] objection . . . and say that there is no *a priori* basis for saying that healthier "*surviving*" children ought not to die more than less healthy children. To support this they have inserted the word "*surviving*" . . . '

Now what can any reader derive from such a sentence? Only that the word 'surviving' was inserted by us after reading Sir Victor's criticisms! Yet here are the actual words of the first edition of our paper :

' Further the higher death-rate of the children of alcoholic parents probably leaves the fitter to survive ' (p. 31).

The text of the memoir has not been altered ; the only addition is a footnote to the word 'survive', saying that we see no reason why a higher death-rate among children of the alcoholic parents is incompatible with better health in their surviving children. Naturally we could only measure the health in the surviving children of school age, and it is these survivors who are of the first importance from the eugenic standpoint. There was no insertion whatever in our second edition of the survival notion ; it was clearly stated for all to read in the first edition.

Now the fact I want to emphasize is this : that Sir Victor

<sup>1</sup> Italics are mine.



Horsley and Dr. Sturge—who in their book have pledged themselves to the great influence of the toxic effect of parental alcoholism on the offspring—have directly twisted our statements in a way which could only be of service to their views if they were appealing to an audience who had not read, or will not read our *Memoir*. We clearly and directly attributed the higher death-rate of the children of the alcoholic parents to a *differential* home environment—to greater carelessness and accident in the drinking families. Sir Victor and Dr. Sturge, notwithstanding this, thrust upon us the statement that ‘alcoholism causes no appreciable detriment to the drunkard or to *his children*’. That is to say, they make a statement, which, if they had read our *Memoir*, they could only make to mislead those who had not read it, namely, that we asserted that the differential environment was no detriment to the children. If on the average the man who uses alcohol is physically stronger than the teetotaler, it is quite possible for his children to be healthier and yet have a higher death-rate, and Sir Victor’s statement at the Medical Temperance Breakfast on this point was not a serious treatment of the subject.

Sir Victor Horsley and Dr. Sturge, without proper quotations from our actual remarks as to defective home environment and the gross carelessness which may be associated with it, says that our suggestion that the greater number of deaths of the children of alcoholic parents arises only in a minor degree from toxic effect is erroneous, because the *accident* rate among young children is not sufficient to account for it. It will be noted that our critics again modify our conclusions and restricts to accidents, presumably followed by fiscal inquiry (there are no inquests in Scotland), the general statements we have made as to accidents, gross carelessness, &c., or the general defects of

home environment. Now let us take out of our Table LVI the deaths of children whose mothers drink or bout. These deaths amount to 529. As we have seen they are about one-fifth more numerous than the deaths among the children of sober parents. Hence we have to account for 106 extra deaths among the children of alcoholic mothers. Now the deaths are those of *all* children of alcoholic mothers, the average age of the children at school being between nine and ten. In some of these families were babies just born, in others adult children, the death-rate covers the *whole* family, of which the average child at school is nine to ten; in other words, the average family will include children at least from four to fifteen years. It seems accordingly reasonable to suppose that the death-rate of our group of alcoholic mothers contributes an extra 7 to 10 deaths per annum due to our 200 alcoholic mothers. That this 7 to 10 deaths is an impossible number to attribute to the differential home conditions we do not believe, and the fact that according to Sir Victor only 17 deaths per annum due to accidents and gross carelessness come to the knowledge of the police in Edinburgh is, in my opinion, not in the least to the point. The mother who retires to the public-house leaving a bacilli-loaded dummy teat in the mouth of her child, or exposes it to the cold and rain by taking it with her, may, owing to her carelessness,<sup>1</sup> destroy a healthy child, but it will not be the subject of police inquiry, any more than are the many accidents the mortal result of which does not arise for months or even years later. But I wish to emphasize the fact that 7 to 10 deaths per annum are what we have

<sup>1</sup> In the 'Introductory Note' of the *Report*, the authors speak of the lurid light the police were in certain cases able to throw on the lives the children led owing to the criminal carelessness of their parents.

to distribute between accidents, carelessness, neglect, *and* toxic effect due to the alcoholism of our 200 alcoholic mothers. Sir Victor Horsley claims the bulk of them for toxic effect. I wonder how many workers among the poor would agree with him! But Sir Victor Horsley and Dr. Sturge appear to have made a gross blunder in citing police returns as to the deaths of infants—possibly because they are not acquainted with the Scottish system. They deliberately asserted that the number of accidental deaths of children below fourteen years occurring in Edinburgh during the last seventeen years was 17 per year, and of these only 5 to 6 deaths per year occurred from suffocation, burns, and scalds, causes which might probably be the direct result of domestic carelessness. They give no annual data with exact causes in each case, but appealed to figures—not public property—said to be prepared by Deputy Chief Constable J. Chisholm and Detective-Sergeant McCondach (*B.M. J.*, Jan. 14, 1911, p. 76). It was perfectly open to them to have used the published figures of the Registrar-General for Scotland. Now from 1890–9 inclusive the total deaths in Edinburgh among children under ten years of age from ‘accident and negligence’ were 574 in number or 57.4 per year; those from suffocation alone are 316 or 31.6 per year. For the remaining nine years, 1900–8, the total deaths from like causes were 532 or 59.1 per year, with 255 from suffocation, or 28.3 per year. The total nineteen years, which cover the period of the Edinburgh Report much more accurately than Sir Victor’s spurious data do, show deaths of children from accident and negligence under ten (not under fourteen years as Sir Victor takes them) amounting to 58 per year, of which 30 per year fall to suffocation alone. These are to be put against the 17 per year, with 5 to 6 from suffocation, burns, and

scalds, which Sir Victor and Dr. Sturge assert to be all that Edinburgh produces. The Registrar-General's returns are precise, exactly where Sir Victor's and Dr. Sturge's are vague and misleading. Further, these critics actually state when driven into a corner that 'it would make no difference whether the parental alcoholism killed the children by negligent accidents or by toxic consequences' (*B. M. J.*, Feb. 11, 1911, p. 336). Considering that they cited their wholly erroneous statistics to demonstrate that my explanation that the higher death-rate was due to negligence and not to toxic effect, their appreciation of logic must be of a curious character.

Here are Sir Victor Horsley's statement and the actual facts side by side:—

*Deaths from accident and negligence per year: Sir Victor Horsley and Miss Sturge: 5 to 6 from suffocation, burns, and scalds. Registrar-General: 22.5 from overlaying alone, 11.5 from burns and scalds alone; total 34, against Sir Victor's '5 or 6'.*

Such is the accuracy of these critics who prattle about others 'imagining and publishing statistical data where none exist in reality'!

They even assert that the large majority of their 17 cases per year 'were cases of children run over by tramcars and lorries in the streets',<sup>1</sup> and suggest that these vehicular accidents were not those of 'helpless ages, i.e. below six years', children needing supervision. Well, the Registrar-General tells us that in the nineteen years (1890-1908) only 5 deaths per annum were caused by vehicles and horses, i.e. only about one seventh of those caused by overlaying, burns, and scalds. Vehicular accidents are not in a 'large majority' at all, and of such accidents 60 to 70% happen

<sup>1</sup> *Times*, January 19, p. 12.

to children under six years of age, and 30 to 40% to children from six to ten! What criticism can be made of persons who simply issue dogmatic statements having no basis in fact at all, except to say that they place themselves wholly outside the court of science?

Why indeed children killed by tramcars and lorries should not be included, even if their numbers were greater than they are, it must baffle any one but a Sir Victor Horsley to determine. Cases of children under five running about the streets without proper control are as much instances of parental carelessness as burns or scalds, and we show in our memoir that the children of drinking parents spend much more of their time than those of the sober in the streets.

As I have indicated above, for a death which is really due to carelessness to be returned as an accident, it must take place soon after the accident, and when the signs of it are recognizable, or the accident must occur in a public place. It is clear that returns made to the Registrar-General do not all come to the notice of the police, or at any rate Sir Victor Horsley and Dr. Sturge had not accurate information from their police informants. Why they choose to use unpublished private data, and to ignore the usual official sources of information, which curiously enough give the number of accidental deaths inconveniently large for their argument, is a question for them to answer. Their verbal quibbles in the *B. M. J.* of Feb. 11, 1911, form no reply acceptable to any one in the least conversant with statistics.

I now turn to the subject which Sir Victor Horsley and Dr. Sturge assert to be

‘the chief generalization raised by Miss Elderton and Professor Pearson, namely, that the effect of his alcoholism on the male parent himself is for biological and social statistics a negligible quantity, since their calculations



showed them that his "physique", "intelligence", "wage-earning capacity", and "efficiency", are at least equal to that of a sober parent' (*B. M. J.*, p. 77).

This statement is an entire perversion of the whole wage question as discussed by us. It is an example, very apt, of what, citing from Herbert Spencer, Dr. Hyslop in his address to the Society for the Study of Inebriety the other night termed the child making a hopeless tangle of a skein of silk. The unwary reader will hardly realize that this question of wages occupied just thirty-three lines on p. 4, and three lines on p. 31, of the original memoir of forty-six pages! It was no 'chief generalization' at all, but was used merely as a supplementary investigation to settle a doubt which arose early in our minds. When we were at an early stage of our work, we confidently expected to be able to demonstrate a sensible influence of parental alcoholism on the mentality and physique of the offspring.<sup>1</sup> But we determined to guard against possible sources of error arising from a differentiation of our two classes of parents *ab initio*, i. e. before the outset of the drinking. If any critic could show that the alcohol using section of our inquiry belonged to a feebler stock *ab initio*, i. e. before they took to alcohol, then the inferior health of their offspring might not be due to their parents' alcohol, but to their parents' stock. To discover whether the parent originally belonged to an inferior stock was the fundamental purpose of our wage inquiry. Any one who will read p. 4 of our original memoir must perceive how this point, not a study of the economic value of the alcoholic, was the source of our inquiry. The following words will explain the whole position :

<sup>1</sup> See p. 31 of our *Memoir*. It was the absence of this sensible influence which led Miss Elderton in her lecture to use the word 'startling', which appears to amuse Dr. Sturge and Sir Victor Horsley!



‘It might even be argued that if drinking parents were physically and mentally fitter than sober parents, the equality in the physique and mentality of children of both types of parents was due to the alcohol pulling down to the average a child who should have been above the average. The point may seem at first sight an unnecessary one to raise, but therein lies really a vital question to the student of modern statistical methods: what are the correlations of physique and intelligence with drinking habit? . . . The only light that can be thrown on this matter from our present data is an indirect one. The wages of the father are to some extent a measure of the general status as to physique and intelligence of the parent. A man who is physically and mentally unfit will hardly receive high wages, whether he be drunk or sober. *Clearly the general tendency to drink must, when it reaches a certain intensity, tend to lower a man's wages.*<sup>1</sup> We should therefore expect to find the wages of the drinking man somewhat less than those of the sober man. . . . We think it may be safely affirmed that if the alcoholic parent were markedly inferior in physique or intelligence, his average wages would be markedly less than those of the sober parent.’

Now I think any fair-minded reader of our *Memoir* will see exactly what we were aiming at: i. e. to ascertain whether, *apart from his alcoholism*, the alcoholic parent was initially of as good stock both as to physique and ability as the sober parent, whether he belonged in fact to the same class in the community. Now our words accurately indicated what we were seeking—and it is a point that our critics appear to have failed entirely to realize—we wanted some proof that the germ-plasm of the drinking section was from the standpoint of mentality and physique neither superior nor inferior to that of the sober. Professor Marshall argued that our drinking section was *superior* to the sober section, they belonged to better stock, who, owing to alcohol, had sunk

<sup>1</sup> Notwithstanding these words Sir Victor asserts that it needed Prof. Marshall's polemic to convince me that the drinker would have lower wages (*B. M. J.*, Feb. 11, 1911, p. 335).

to 'low grade' jobs, and because they were of superior stock they had children physically and mentally as fit as the sober stock. We wonder what place this leaves for Sir Victor Horsley's marked toxic influence! Yet Sir Victor Horsley cites Professor Marshall with approval, in all probability merely because the latter has criticized our *Memoir*. To refute Professor Marshall's position it was needful to show that the drinkers did not follow 'low grade' jobs. Accordingly, a list was formed of all the trades, and it was at once obvious that the sober and the drinking sections were scattered through all trades alike, and that the latter were not concentrated on 'low grade' jobs.<sup>1</sup> Of this list of trades I shall have something to say shortly.

The question then arose as to some *rough* classification of these trades, and the only method of classifying them according to the ability and physique needed for their pursuit seemed to be that of classifying them according to the current wages of the trade in the district. In doing this it was not material to determine whether the drinking workman was in receipt of the full time wages of the trade. The question was: Had he selected a trade which required ability and physique? The trades followed by the drinking section, as judged by the wage standard, were found to be on the whole those requiring the greater intelligence and strength—they were not 'low grade jobs'. When the drinking workman chose his trade—in the great bulk of cases before the question of alcohol had become crucial—he chose the higher class of employment. The reader who will carefully study our attitude in this matter will see how absurd it has been for Sir Victor Horsley and Dr. Sturge to quote the trade wages as cited by us from a table of them given in the *Edinburgh Report*, as if they were and

<sup>1</sup> *Questions of the Day and the Fray*, No. I, p. 10. Dulau & Co.

we had used them as a measure of the wages of either sober or drinker. They were solely the current wages of the trade by which the trade itself was roughly graduated as a 'high' or 'low-grade job'.<sup>1</sup>

Now consider the following paragraph penned by Sir Victor Horsley and Dr. Sturge:

'It will be seen from the comments we have extracted from the Edinburgh schedules that Professor Pearson has bestowed a wage of 25s. 6d. per week on fourteen individuals concerning whose real wages either nothing is known, or that, whatever they may earn when they were at work, those industrious occasions were so rare (and sometimes rendered so impossible by the man being in prison) that no average weekly wage could be accurately ascertained. Nevertheless the exigencies of Professor Pearson's argument are great and he accords to each of these individuals 25s. 6d. per week' (*B. M. J.*, p. 79).

Or, again, speaking of a father in an asylum, Sir Victor Horsley says that he is

'represented by Professor Pearson in his table to be earning 26s. 6d. a week' (*B. M. J.*, p. 74).

Illustrations of this sort of argument abound in Sir Victor Horsley and Dr. Sturge's paper. *They are wholly and entirely false charges.* The average trade wages were used to graduate trades, the average wages obtained by drinking and sober parents *were ascertained directly from the whole bulk of wages* given in the *Report*, and *not* as Sir Victor and Dr. Sturge insinuate from the average trade wages. Now let us see exactly what our *Memoir* states as to wages (p. 4):

'Parents were divided into three classes: (1) both parents drink, (2) one parent drinks and (3) neither drink. The

<sup>1</sup> Had we classified trades as 'high' or 'low' according to their general mortality rates, we should probably have been told that we had asserted that all individuals, temperate or intemperate, died at these rates!

mean wage of the father when both parents drink is 24s. 8d.; when one parent drinks, 25s. 6d., and when neither parent drinks, 25s. 5d. Or, grouping in another way, when either or both drink, 25s., and when neither drink, 25s. 5d.'

What conclusions did we draw from these results? Not the 'chief generalization' asserted by Sir Victor Horsley and Dr. Sturge, that the wage-earning capacity and efficiency of the drinker are at least equal to the sober, but that the wages of the alcoholic are 'wholly inconsistent with a marked mental or physical inferiority in the alcoholic parent' (p. 31).

In other words our object was, not to show the efficiency of a workman in a state of alcohol ('clearly', we had said, 'the general tendency of drink must when it reaches a certain intensity be to lower a man's wages') but to show that the drinking workman was not *ab initio* and apart from his alcohol inferior physically or mentally to the sober. Now this is the very point which Sir Victor Horsley and Dr. Sturge will themselves have to demonstrate before they can deduce any result from statistics of sobriety and intemperance. Until you have demonstrated general equality of the germ-plasms of the two stocks as far as physique and mentality go, it is impossible to assert that a differentiation of the offspring is due to alcohol. That they have not recognized the importance of this point is a measure of their fitness to deal with such complex statistical problems.

Now Miss Elderton has been good enough to go through the whole of the wages in the *Report* again. She has divided the workmen into five classes: (i) Those in regular employment. (ii) Those in irregular employment but whose average wage was not given; in this class were included those who were stated to have been out of work or idle, or for whom 'wages when working' were given. (iii) Casual

employment; this class contains those who follow trades with seasonal employment as well as the jobbing workers. (iv) Average wage class, those with irregular employment whose average wages are stated. (v) Those with no occupation or employment, being idle, ill, in prison, or incapable of work.

Now the results are embraced in the table below. Personal judgement will of course arise in classification, but this new table has been formed in perfect independence of the former table, and the general results are in close agreement.

Class.	Sober.		Drinking.	
	Number.	Average Wage.	Number.	Average Wage.
(i) Regular . .	167	27·1s.	163	26·1s.
(ii) Irregular . .	17	27·1s.	29	27·0s.
(iii) Casual . . .	33	22·7s.	79	24·0s.
(iv) Average . .	34	26·1s.	46	23·6s.
General mean	251	26s. 5d.	317	25s. 4d.
(v) No work . .	20	0	35	0

Now this table shows several points: (*a*) that when in regular employment the drinker is worth 1s. a week less than the sober. This is exactly what we stated in our *Memoir*; (*b*) that in casual or irregular labour he gets paid the same or slightly more; but (*c*) that on the whole he is more frequently out of employment, as evidenced by the average wages when given. The general mean of all the wages is 1s. 1d. less for the drinking section than for the sober. Now these wage data will be found more fully discussed in a paper just published in the *Journal of the Royal Statistical Society*,<sup>1</sup> but they seem to us to demonstrate that our original point was correct, namely, that the wages do not indicate that the drinking workman belongs to an

<sup>1</sup> Vol. xxiv, p. 221.



inferior stock either mentally or physically; he is a man who independent of alcohol either does or could earn wages equal to those of the sober man. This was the inference we drew from the data in our original memoir, and not the 'economic equality' of the two classes of labour. It is always possible to tangle the 'skein of silk' by picking out isolated sentences or words and wilfully overlooking the context.<sup>1</sup> Sir Victor and Dr. Sturge assert that the chief statistical generalization of the paper was the statement 'that the wage-earning capacity and efficiency of the drunkard is at least equal to, possibly a little above, that of the sober workman' (*B. M. J.*, p. 70). When it is pointed out that so far from being our 'chief generalization', we have made no such statement at all, they reply by quoting our words: 'On the whole it seems reasonable to assume that the drinking parents are in physique and mentality the equal on an average of the sober, or little above their standard' (p. 5 of *Memoir*, cited by Sir Victor Horsley and Dr. Sturge in their letter to the *Times*, Jan. 19, 1911), but they do not say that the reason for this statement lies in the fact which we have stated in the lines above it, that the wage-earning power and efficiency

<sup>1</sup> Thus in *B. M. J.*, p. 77, Sir Victor Horsley and Dr. Sturge deliberately quote what they say are contradictory results as to wages, although the exact meaning is given on page 4 of our paper—25s. as the wage of a drinking *man*, 26s. as the wage of a sober *man*, 25s. 6d. as the wage where *one parent* drinks, 25s. 5d. where *neither parent* drinks. The former refers to the wage looked at from the standpoint of the man, the latter as the wage from the standpoint of the *child*, i.e. the conditions as to wage from the standpoint of *each* child according as both, one, or neither of its parents drank was recorded. The exact meaning of the wages cited in the text of the *Supplement* is stated in a footnote and agrees absolutely with the *Memoir*. Verbally the wording in the text was careless, but what shall be said of critics who disingenuously put on one side an explanatory footnote, which states exactly what the 25s. 6d. and 25s. 5d. were, i.e. the mean wages when one *parent* drank, and the mean wage when neither drank. They were the wages reached by a classification of the parents of each child.



was less, namely, about 1s. a week, and that this difference did not seem to us to mark a widely inferior physique or intelligence in the drinking section, but to connote the employer's dislike for the inconveniences of inebriety. That view was supported because we knew that when employed in irregular work or casual and seasonal trades the drinker got rather higher wages than the sober. Our point was not, and never has been, that a heavy drinker does not weaken his physique or mentality,<sup>1</sup> but that our drinking individuals were not initially and apart from their alcohol of inferior physique and mentality. I believe that Sir Victor Horsley and Mr. Keynes are labouring this point of wages, because they have accepted, without full knowledge of modern theories of heredity, the view that somatic variations produced by habit or environment are at once influential in changing the character of the germ-plasm.

I now turn to the tables which Sir Victor Horsley and Dr. Sturge have extracted from the Edinburgh records, and contrasted with what they assert are *our* results. First let us take *Teetotalers*. Our critics give a table of nineteen teetotalers and their wives, and proceed to suggest that these nineteen persons have been used by us without any consideration at all. The exact number of teetotalers used by us is fourteen fathers and thirteen mothers. This is given in the *Memoir*, but Sir Victor and Dr. Sturge at once attribute to us five teetotal fathers whom we have not called teetotal but alcoholic! Further, the Edinburgh records are very full as to the past history of drink whenever there has been one, and absence of this history may be taken as a sign that the family has been a steady and sober one. Further, when no statement as to drink is

<sup>1</sup> We have made no investigation of this point.

made the Secretary informed us, before starting the work, that we might consider the parents as sober, or that the Committee's inquiries, in most cases from police, missions and employers, had produced no evidence of drink.

The following is Sir Victor Horsley and Dr. Sturge's *List of Teetotal Fathers*:—

<i>Index No.</i>	<i>Habits of Wife. Horsley &amp; Sturge.</i>	<i>My remarks, with extracts from the Edinburgh Report.</i>
22	No evidence.	Included by us among the drinking fathers, because he had been 'a drunken wreck till two years ago'. Not included in table of families, because the wife is dead and there is no mention of her habits.
57	Teetotaler.	All the family are Good Templars; no history of drinking. Included by us in the group 'neither parent drinks'.
93	Probably a teetotaler.	Why 'probably'? The report says, 'Decent, spotless clean house and family; teetotal and well-doing. Included in 'neither parent drinks'.
108	Teetotaler.	No history of drink. Included in 'neither parent drinks'.
141	No evidence.*	No history of drink, and therefore wife was sober, the husband was a strict teetotaler for the last twelve years of his life. The account does <i>not</i> state that he drank before this, only that he became a <i>strict</i> teetotaler. 'In our opinion', write Sir V. H. and Dr. S., 'the father's parentage of these children was not, strictly speaking, a teetotal one, but an alcoholic one, in diminishing ratio'. The first child of whom we have details is 'dull', and Sir Victor Horsley tells us that it 'was born immediately after the father's teetotalism commenced, and the three others followed within five years'. The child was born $2\frac{1}{2}$ years after the father became a <i>strict</i> teetotaler: there is no evidence that he drank before that, and the youngest child 7, not 5, years after the <i>strict</i> teetotalism commenced.
147	Dead nine years.	Included as a drinker because he drank hard till his wife's death. Not included in any group of both parents, because his wife was dead and no details as to her habits given.
200	No evidence.	Incorrect statement made by Sir Victor and Dr. S.: Father still drunken although he has taken the pledge. Therefore not included in our table of teetotalers.
244	No evidence.*	No history of drink. Wife sober. Erroneous statement of Sir V. H. and Dr. S.

<i>Index No.</i>	<i>Habits of Wife. Horsley &amp; Sturge.</i>	<i>My remarks, with extracts from the Edinburgh Report.</i>
258	Teetotaler.	No history of drink. 'Father most excellent, trustworthy and first-rate'. 'Very nice, well-doing teetotal people'. Included in 'neither parent drinks'.
260	Drinks.	Father teetotal; no past history of drink. Mother drinks a little. Case went of course into 'Mother only alcoholic'.
336	Drinks, but not seen to be drunk.	This is a good illustration of Sir V. H.'s method. The statement is, 'A nice family, the man teetotal and steady at his work, the wife not teetotal, but never seen the worse for drink'—'the home is a comfortable one'. No man would have an objection to being described as 'not teetotal, but never seen the worse for drink'. He would properly bring an action for libel if described as 'drinks, but not seen to be drunk'.
342	Teetotaler.	No history of drink. 'A model family'. Entered by us as 'neither drink'.
367	Teetotaler.	No history of drink. 'Strict teetotalers'. Entered as 'neither drink'.
479	Teetotaler.	No history of drink. 'Strict teetotalers'. 'Drinks an enormous amount of tea and looks like it'. Entered as 'neither drink'.
530	Drank at least during child-bearing period.	No history of drink in case of father. Wife began to make free with drink at one time, but took pledge and is all right. 'Nice family'; 'Good workman; rent always paid up to date'. Entered under 'Mother only drinks' by us, although this is scarcely justified. Sir V. H.'s statement that she 'drank at least during child-bearing period' is still more unjustifiable.
621	Suspected of drinking.	No history of drink in case of father. Entered by us under 'Mother only drinks'.
643	No evidence.	'A total abstainer and man of splendid character'. Entered as father does not drink, but not in the double parent tables, as there is no statement as to mother's (earlier wife's) habits.
715	No evidence; * home miserable.	This man should not have been entered as a teetotaler by Sir V. H. and Dr. S. He has a past history of drinking. His wife has no such history and may therefore be treated as sober. She is a slattern.
774	Teetotaler.	No past history of drink. 'Respectable, sober people'; 'total abstainers'.

\* In cases marked with an asterisk in this table Sir Victor Horsley and Dr. Sturge have made a mis-statement in saying there is 'no evidence' as to habits of wife.

Now it will be found that this table just gives the fourteen fathers we have reckoned as teetotal, and we find nothing whatever to withdraw from our classifications. But the table illustrates Sir Victor Horsley's methods most accurately! Thus he writes:—

'No. 200 figures as a teetotaler, but he only "recently" signed the pledge after a "reckless" life. His youngest child was  $6\frac{1}{2}$  years old, however.'

The suggestion made is that we have included the case as that of a teetotaler, and Dr. Sturge and Sir Victor proceed, 'As Professor Bateson truly observes of Professor Pearson's work, it is very obvious that "his methods dispensing with individual analysis of the material are useless".'

Sir Victor says that No. 200 only recently signed the pledge; the *Edinburgh Report*, p. 10, gives him as still drunken, a point which Sir Victor overlooked with *his* individual analysis, but which led us to classify him still among the drinkers.

No. 147 is another illustration of Sir Victor's method. The case is one included by us among the drinkers. He holds the case up as an example of what absurd classification we have made of the teetotalers, and adds as comment:—

'No doubt Miss Elderton and Professor Pearson have saved themselves much time and trouble. but only at the expense of their own scientific reputation and that of the Galton Laboratory of Eugenics.'

He then tells his readers that

'We shall presently see further inaccuracies and errors, for which these authors are responsible, owing to their omission of necessary statistical precautions in examining the data they selected and their method of creating such statistics where real data did not exist.'

He thus directly charges us with forgery on the basis of inaccuracy and errors which he has himself insinuated are

in our investigations, but which have no existence in our classification. There is only one word by which this process can be described, but it is better left unsaid.

Let us now fix Sir Victor Horsley and Dr. Sturge down to another of their characteristic statements :

‘To return now to other kinds of porters—namely, porters, coal porters, and street porters—we have put them together in one table as below. Of porters thus grouped we find only thirteen in the Edinburgh statistics, and no other porters are mentioned therein. Professor Pearson, however, says there are seventeen. He, therefore, has invented for this trade four imaginary individuals, although asserting throughout that he is quoting the Edinburgh figures. The Edinburgh Committee verified the wage rate, showed that for eleven individuals this could be done, and the average weekly wage came out at 20s. 6d. Of course Professor Pearson, to construct his wage (and sobriety) table for the *Times* and his *Supplemental Memoir*, gives this average weekly wage of 20s. 6d. to all his seventeen porters, the thirteen real ones, and the four imaginary ones, in spite of the Edinburgh data. Turning now to the sobriety question, Professor Pearson makes out of his seventeen porters, nine were sober and eight were drinkers, each of whom gets according to him 20s. 6d. Thus for this relatively low wage he naturally obtains by this “expert” method a practical equality between the wage-earning of the drunken and of the “sober” porter respectively’ (*B. M. J.*, Jan. 14, 1911, p. 81).

The above is a typically Horsleyan sample of controversial writing! No statement as to the wage-earning of sober and drunken porters has ever been made by me. No average was struck for such porters by ‘expert’ or any other methods. In my letter in the *Times* and in the *Supplemental Memoir* a list of trades was given, and to grade these trades as high or low a list of the wages paid in Edinburgh for these trades was attached to them, taken from the Edinburgh Committee’s Report. No statement



was made that each of the seventeen porters got 20s. 6d., but the statement was made that porters followed a trade the grade of which was 20s. 6d. as compared, for example, against masons with a 34s. 1d. trade. It was perfectly clearly stated that the wages given were average trade wages and used to grade the trade and not the man. The object was, of course, to ascertain whether the sober and the drinking workmen followed the same trades, or the latter had, as Professor Marshall asserted, sunk to 'low class jobs'.

But there are still more typical examples of Sir Victor's accuracy in the paragraph cited. He finds that I have seventeen porters, and in his own superficial examination of the *Report* he only discovers thirteen. He then at once starts with accusing me of creating imaginary porters. This is markedly characteristic; instead of inquiring what has been done before he makes a gross blunder and an absolutely unjustifiable assertion, he rushes into print with his 'imaginary porters'. Now the rule of our Laboratory, and I believe of most trained statisticians, when dealing with trades and classifying for small numbers is to adopt the Registrar-General's scheme—in this case identical with that of the Registrar-General for Scotland. The Registrar-General's group contains *Messenger, Porter, &c.* (*not Railway*) and he uses this in his table for the *Effects of Alcoholic Excess*.<sup>1</sup> Are then the 'four imaginary individuals invented for this trade' the messengers customarily included by statisticians with porters? Not at all, only two of them have this origin, namely:

No. 341. Messenger, in regular employ, wage 21s. and excellent character for steadiness and trustworthiness.

<sup>1</sup> *Supplement to the Fifty-Sixth Annual Report, Part I, 1908, p. cix, and also Instructions to the Clerks employed in Classifying the Occupations and Ages of the People. England.*



No. 410. Messenger, wage 30s. No history of drink ; gambles, said to have acquired his property thus ; wife and he said to have been before the court for shebeening and gambling.

The other two 'imaginary porters' exist in the record all right, *only Sir Victor has taken no trouble to examine the record accurately !* His careless examination not providing them, he accuses me of inventing them ! They are

No. 217. Fish porter. Wages 18s., in regular employ. Fits of drinking.

No. 170. Porter. Wages 22s., in regular employ. No sign of poverty here ; house tolerably clean and comfortable. Appear to be fairly temperate.

Sir Victor Horsley's own list describes thirteen porters, six sober and seven drinking ; adding the above four cases we have nine sober and eight drinking, numbers absolutely identical with those of the statement originally made by me.<sup>1</sup>

<sup>1</sup> It would only weary the reader and serve no useful purpose to go through trade by trade and show that Sir Victor Horsley's criticisms are precisely of the character of the above. Thus, he says, that I have made 'twelve coopers, of whom eight are "sober" and four drunken, and that all these twelve earn 28s. 6d. a week, whereas the actual schedules of the *Report* give, as we show in the above table, only eight coopers of whom seven are "sober" and one drinks'. Of course, I have made no such assertion that each of my twelve coopers earned 28s. 6d.; it is a perfectly gratuitous misstatement on the part of Sir Victor. What I have done was to state that coopers rank as a 28s. 6d. trade. As for the four 'imaginary' coopers with which Sir Victor credits me, they arise from including, as the nearest allied trade, four isolated individuals engaged in cooper's work, in wood-cutting, splitting, and turning (No. 295, regular 30s., seems steady ; No. 516, regular 20s., drinks ; No. 649, 29s.-37s., drinks, out of work ; No. 714, regular 25s., very drunken). These give precisely the eight sober and four drunken of our table. The mean wage value of these four men—apart from drink—is 27s. 6d., and they belong to the Group B of my classification of trades, e.g. wages between 25s. and 30s., and this is where I placed them in my letter to the *Times*, where the only question was what class of trade did they follow. In the quite different problem

A few further illustrations of Sir Victor and Dr. Sturge's perfectly random misstatements may be extracted from their letter of Feb. 11, 1911 to the *B.M. J.*<sup>1</sup> Take the following sentence :

'If your readers have the curiosity to turn to this [the paper in the *Royal Statistical Society's Journal*], they will find a most extraordinary compendium of estimates as to how much the drinker and the sober man respectively earned, but not from beginning to end does he mention the original estimate published in the first memoir. This we may suppose, therefore, will also be buried, like various other inconvenient conclusions to which we have drawn attention' (p. 335).

The 'compendium of estimates' is identical with that given above. That Sir Victor thinks it 'extraordinary' probably arises from his total ignorance of the methods of statistics—it is probably the only form in which statistics of wages in a population of any kind can be presented. So far from the original estimate being 'buried', the table practically confirms the original estimate and accords closely with that estimate, which indeed was formed essentially in the same way, with a little less rigid definition of categories, and so a possibility of more personal equation. The result shows that the possibility was not an actuality.

of individual wages, three of them have been classed as regular according to the record, and the fourth in the group of no work of our Table, p. 21 of this paper. Notwithstanding this, here as elsewhere, Sir Victor Horsley makes the deliberate misstatement that I have asserted that 'all these twelve earn 28s. 6d. a week'. Absolutely the same type of misstatement occurs in his remarks as to masons (our classification including bricklayers—a small body in Scotland having no separate union, and combined with masons by the Registrar-General), as to painters, &c. It is purely idle, however, to waste time in going through each such case in detail.

<sup>1</sup> The Editor of that Journal thought any reply on my part to the nineteen columns of Sir Victor and Dr. Sturge unnecessary, and I only succeeded under considerable difficulties in getting my letter of Feb. 4 inserted. A brief reply of a few lines to the above letter was acknowledged by the Editor but not inserted. Comment is needless.

But the remarkable point is that the following words occur in my letter to the *Journal of the Royal Statistical Society* immediately after the 'compendium of estimates' :

'This is the basis of the statement made in the *Memoir* that the 1s. difference shown in the wage results might reasonably be supposed to indicate an employer's preference for sobriety, and did not mark an inferior physique or mentality in the alcoholic section.'

To this are appended the words :

'The values given above agree substantially with the values 26s. and 25s. given for the sober and drinker on p. 4 of our original memoir, which Mr. Keynes misquotes in the paragraph I have cited above. The 25s. 6d. and 25s. 5d. are the wages when one or other parent drinks and when neither drink, and apply to families with both parents alive' (p. 228).

Now Sir Victor states that 'not from beginning to end does he [K. P.] mention the original estimate published in the first memoir' which Sir Victor states that he supposes will now 'be buried'. We have here another essentially striking instance of Sir Victor's method of controversy. He writes for an audience which he apparently assumes have not seen and will not see my paper, and he then deliberately states that a result which really confirms the first statement as to the wages of the sober and the alcoholic, makes no reference to that statement 'from beginning to end', and he further suggests that that statement, like other 'inconvenient conclusions', is 'buried'. The main point of interest is, of course, that this very 'inconvenient conclusion', so far from being buried, was substantiated by a fuller inquiry, and the inquiry definitely stated that it was so on the very page Sir Victor professed to cite!

I said in my letter to the *B. M. J.*, Feb. 4, 1911, p. 280, that the meaning of the word parent seemed to be quite

overlooked by Sir Victor. To this Sir Victor and Dr. Sturge reply :

‘We are not afraid of your readers thinking that we do not understand the meaning of the word parent, but we would point out that even a divagation of this sort is of no use to Professor Pearson, because the original estimate of Miss Elderton and himself which is now relegated by him to the dark ages, referred to “fathers only”’ (*B. M. J.*, Feb. 11, 1911, p. 335).

Now here, again, is one of those extraordinary misstatements on the part of our critics, which might lead us to credit them with an intellectual confusion amounting to unscientific obsession! Here are the exact words of our ‘original estimate’ (p. 4):

‘Parents were divided into three classes: (1) both parents drink, (2) one parent drinks, and (3) neither parent drinks.’

That is to say, not each family but each child was entered on the table into one or other of these three classes, and against the child was put its father’s wages; but to continue:

‘The mean wage of the father when both parents drink was 24s. 8d.; when one parent drinks 25s. 6d. and when neither parent drinks 25s. 5d. Or, grouping in another way, when either or both drink 25s., and when neither drink 25s. 5d. If we consider the father alone, for it is not possible to apply the wages test to determine the drinking mother’s status in comparison with other mothers, we find that the wages of a drinking man are on the average 25s., and a non-drinking man 26s.’

Now, in the first place, I observe, that the estimate in our original memoir does not concern ‘fathers only’ as Sir Victor asserts, and that the contrast is actually drawn in the original memoir between ‘parent’ and ‘father’. Here in the first memoir we have the basis of the ‘divagation’, which Sir Victor still fails to grasp, because he has

not seen what is meant in the above paragraph by 'parent' and what by 'father'.

In the first investigation *each* child of whom we had records was taken—there might be two or three from the same family—and the question asked, is the home a drinking home or not? It was then found that where both parents drunk the wage was least, and that when neither parent drunk the wage was a penny less than the case where either parent, *father* or *mother*, drunk. The object here was to ascertain whether these stocks in which *either* parent drunk were from the measure of the father's wage *a priori* of lower type. This was clearly not the case. In the second investigation the individual workers were taken and their wages led us to the conclusion that a sober man was worth 1s. a week more than the drinker. There is, as far as I can see, no confusion or difficulty about these results at all. The one applies to drinking in the home by mother or father and allows for the differential fertility of the sober and the drinking; the second is a result applying only to male workers, and is not affected by the number of their offspring or the question of whether their wives drink or not. Which of these two estimates ought to be used depends on the subject under discussion; when dealing with the influence of parental alcoholism on the child, and ranking the mentality and physique of the stock from which it springs, the first seems to me the more important measure.

One more illustration of Sir Victor's misstatements from the letter of Feb. 11 (*B. M. J.*, 1911, p. 335) may be taken. Referring to my table of classification of trades, he writes:

'Professor Pearson actually says in his letter on February 4th that his table "has nothing to do with sobriety or intemperance". If this were true we are to suppose that the headings S. and D. have not got the



meaning which Professor Pearson in his memoir and in the *Times* said they had.'

Now here are the exact words of my letter of February 4 (*B. M. J.*, 1911, p. 279):

'The origin of this wage value of the trade is perfectly well known to Sir Victor Horsley and Dr. Sturge; we stated it ourselves. It is "the average of the *recorded* wages of individuals following that trade", and has nothing to do with sobriety or intemperance; it is a trade scale, and not a sobriety scale.'

Here is the sentence: 'this wage value of the trade . . . has nothing to do with sobriety or intemperance.' Sir Victor deliberately—there is no other word for it—cuts out the noun 'wage value of the trade' and replaces it by 'table', and then proceeds to assert that it is needful to suppose that my headings S. and D. do not refer to 'sober' and 'drinking'! After this peculiarly surgical operation I think I can afford to leave Sir Victor to the judgement of the future, and to the applause of that audience—somewhat remote from the field of science—for which he seems to crave. Where there is noise and clatter there is no real science in the making.<sup>1</sup>

<sup>1</sup> I do not intend to carry this controversy further, but hope to issue shortly other *Memoirs* dealing with alcoholism. I may, however, state here that I wholly disagree with Mr. Keynes's naïve attempt, although a party to a controversy, to act as judge and sum it up (*Journal of the Royal Statistical Society*, vol. lxxiv, p. 339). No doubt Dr. Saleeby's melodramatic visit to the North Canongate School in Edinburgh (*B. M. J.*, Feb. 11, 1911, p. 332) will be given by medical readers of the *B. M. Journal* just the weight they are accustomed to give to his judgements on medical and other topics. As a layman, I always find the fertility of his imagination and the picturesque flow of his language most impressive; yet they invariably seem to lack the convincing factor which arises from intimate study of any single subject. Of Sir Victor Horsley's attempt to discredit our Manchester Data by stating that in his opinion the material is 'quite useless for the purpose to which they were put', little need be said; no one who has studied the manner in which Sir Victor Horsley and Dr. Sturge deal with statistical material in their

But in now bringing to an end, so far as I am concerned, the present controversy I should like to say a word or two on the bearing of alcohol and the temperance movement on national welfare. The first memoir on alcoholism by Miss Elderton and myself was written wholly without bias and without knowing until the final numerical reductions were

*Alcohol and the Human Body* will give their judgement as to the value of statistical material a moment's consideration (see our *Second Study of the Influence of Parental Alcoholism on the Physique and Ability of the Offspring*, Dulau & Co., 1910). What the late Dr. Ashby actually thought of the value of the case-papers prepared under his supervision is known to, but can now hardly be proven by even his intimate friends; but Dr. Ashby was aware that the schedules were being copied by Miss Dendy with a view to my inquiry as to the possible sources of mental defect; and, when Miss Dendy nearly gave up in despair the heavy labour of copying these case-papers, it was he who persuaded her to persevere in her laborious task. Nobody has asserted that 'he made himself responsible for their full accuracy'. Does Sir Victor Horsley make himself responsible for the full accuracy of the data collected by laymen which with no warning of the use of 'vague and unscientific terms' he cites from MacNicholl or Laitinen? I have in my possession hundreds of family histories, collected not by surgeons but by medical men of every degree of distinction. In nearly all cases they have cited the 'vague and unscientific terms' used by the laymen from whom they obtained their data. But the fact that they have done so does not invalidate their material where used for scientific statistical purposes. It would be better if the ailments of every member of a family, be they tuberculosis, epilepsy, or alcoholism, could be diagnosed by one and the same medical man, but we all know this to be in practice impossible—the medical man himself has to get the family history in the bulk of cases through one or two living lay members of the family. And after all in our present state of knowledge are medicine and surgery such certain sciences that even professional diagnoses are not liable to err? I know of a family in which for three generations its hereditary weakness was recognized in 'vague and unscientific terms' as something liable to go wrong in the gall-bladder; one most distinguished medical man diagnosed a swelling in one member as cancer of the stomach, and persisted in it to the death certificate, although all trace of the growth suddenly disappeared a month before death; a not less distinguished surgeon operated on a second member for appendicitis—I believe he had difficulty in finding any appendix at all to remove—and the patient was passing gall-stones before she was up from bed after the operation. Neither of these gentlemen troubled themselves to inquire into the family history; they were quite certain, and would scorn, like Sir Victor, those 'vague and unscientific' appreciations of the layman, to which wiser and more prudent men give due—but not undue—weight.

made what was going to be the outcome. We had failed to find a marked influence of other environmental factors on the health and mental powers of the child, but we confidently expected to find it in the alcoholism of the parent. Why? Because we had been so repeatedly told it existed by authorities we credited with a scientific training and a scientific instinct. We did not find it, but we found instead a perfectly appalling mass of fanaticism. We have aroused this fanaticism in a remarkable manner to every degree of invective, misstatement and extravagance of rhetoric. We discovered that not only a large part of the lay press, but of the medical press is not open to anything but a one-sided discussion on these matters—it is already in the hands of so-called temperance reformers. Then we studied those authorities whom we had assumed had given energy and time to a scientific investigation of alcohol, and we found that the whole 'scientific' basis of the movement was worthless. The men who were demonstrating the evil to the offspring of alcohol-using parents were doing so in nine cases out of ten by statistics, which they had no more notion how to handle properly than boys in an elementary school. Whether we were right or whether we were wrong was relatively of little importance, compared with the national danger of a movement which has become immensely influential in press and on platform by claiming a scientific basis which it does not possess. The movement is by no means unlike the English Puritan movement of the seventeenth century. The country was then politically and religiously unsound and the Puritan fanatic saw only one way to mend it. Extreme fanaticism led, as it always does, to reaction, and we had all the evils of the last two Stuarts. Only after years of extremes of both types did moderation in politics and religion get a chance. The bulk of the educated and thinking population

of this country has not troubled itself about the temperance movement; it has become sober—not on account of any teaching—but owing first to a change of customs, secondly and chiefly to a change of occupations which has involved a corresponding change of tastes. Nine men out of ten of the middle classes in this country, if asked why they had given up the considerable consumption of alcohol of their fathers, to say nothing of the excessive use of alcohol of their grandfathers, would reply, and in most cases quite truly, that they found no pleasure in the habit. The change of taste—probably associated with a difference of foods and occupations—has been far more influential in producing sobriety in the middle and professional classes than any active propagandism. The result has been that the temperance movement has grown up largely outside the influence of the educated, critical, and scientific factors in our national life; its propagandism is in great part based on statements which, whether true or false, have never been properly tested from the standpoint of science. The educated, thinking man wants to know the truth about vaccination, inoculation, vivisection, and alcohol; he is ready to act on knowledge, but he is met at every turn by rhetoric, invective, and fanaticism, till he thoroughly distrusts ‘anti’s’ of all types. This is a national disaster, for there may be some truth behind any of these causes, and, if there be not, there is possibility of much harm.

I am not one of those who think there is no alcohol problem in this country. Had I thought so, I should not have devoted any of the limited energies and the slender funds of the laboratory under my control to the investigation of this subject.<sup>1</sup>

<sup>1</sup> I think also that it had the general approval of the founder of that laboratory. Sir Francis Galton told me some months before his death that

The general conclusions we have so far reached—and none of the criticism poured out on us in the least weakens my confidence in their general truth—are :

(1) That if the population be divided into the sober and the drinking sections, there is no *marked* influence on the physique and mentality of the offspring produced by the alcoholism of the parents. The toxic action asserted to exist by Dr. Saleeby, Sir Victor Horsley, and Dr. MacNicholl has been grossly exaggerated for the purpose of propagandist effect.

(2) The association of alcoholism with tuberculosis, epilepsy, insanity, mental defect, deformity, dwarfism, &c., in *some* stocks cannot for a moment be denied. The assumption, however, that every association is causation is the rock upon which most of the pseudo-scientific work of temperance fanatics has been shipwrecked.<sup>1</sup> A careful statistical examination has shown us that in cases of extreme alcoholism the insanity or mental defect as a rule antedates the alcoholism. Since writing the *Second Study*

he had written a letter on the subject to the *Journal of the Society for the Study of Inebriety*. It does not appear, however, to have been yet published by the Editor of that journal.

<sup>1</sup> If ridicule could kill want of logic, this desirable end would be achieved by such a letter as that of Dr. Charles Mercier in the *B. M. J.*, Jan. 21, 1911, p. 165. I venture to quote some sentences from it :—

‘My suggestion, that the diminished use of alcohol is responsible for the increase of appendicitis, was made, in common with my other suggestions, as an application of the great principle of causation—*post hoc, ergo propter hoc*—discovered by those keen observers of nature, the schoolmen of the Middle Ages, and adopted with such striking results by medical practitioners in assigning the causes of disease. . . . Teetotalers have taught us to appreciate the lethal powers of alcohol, and to recognize how those who use beer and wine, like those who use fuses :

All grow by slow degrees,  
Brainless as chimpanzees,  
Meagre as lizards;  
Go mad, and beat their wives;  
Plunge, after shocking lives,  
Razors and carving-knives  
Into their gizzards.’



*of the Influence of Parental Alcoholism on the Physique and Ability of the Offspring*, two things have occurred to strengthen our position in this matter. The first is that we have been able to obtain a copy of the original of Demme's investigations cited by Horsley, Hodge, Basil Price, and Kirby, without giving any reference to the original. This shows at once that Demme selected his children of drunkards, not by selecting drinking families, but by selecting children who came to the *Fenner'sches Kinder-spital* on account of mental defect, wanting or imperfect development of speech, or imbecility or idiocy, and that when he found families in which such child-defect occurred he then inquired as to the alcoholism of their ancestry. Further, in a very large number of such cases, on his own showing, the child itself was, owing to the too early consumption of much alcohol, reduced to a condition approaching or actually epileptic. Demme's book, *Ueber den Einfluss des Alkohols auf den Organismus des Kindes* (Enke, Stuttgart, 1891), is written in a perfectly moderate and reasonable spirit. He draws the conclusion that alcoholic drinks should not form part of the daily nourishment of young children; he looks upon alcohol in the case of children as a useful drug to be administered under medical advice, and on quite a different footing to its use by adults.<sup>1</sup> I may take occasion to return to Demme's statistics in another place, but it is quite clear that those who have taken them out of their context, without stating the nature of Demme's selection, nor the lesson he himself has drawn

<sup>1</sup> 'So wohlthuend, anregend, belebend der Alkoholgenuss namentlich als Wein oder Bier für den *fertigen* Organismus, für den *Erwachsenen*, bei anstrengender geistiger und körperlicher Arbeit, erscheint, als so *unzweckmässig* und *nachtheilig* muss er im Sinne eines *gewöhnlichen* Genussmittels für den *unfertigen kindlichen Organismus* bezeichnet werden.' S. 67. The italics are Demme's.

from them have committed from the standpoint of science a very grave offence, however much it may have profited their own propagandism.

The second thing to which I have to refer is the appearance, since the publication of our memoir, of Dr. Wilhelm Stöcker's book, *Klinischer Beitrag zur Frage der Alkohol-psychosen* (Fischer, Jena, 1910). The experience of this book essentially coincides with that of the material collected by the Eugenics Laboratory: The abuse of alcohol is not the cause of mental defect and insanity, it is to be considered itself as the outcome of a diseased mental condition. Dr. Stöcker writes as follows:

‘In der Mehrzahl meiner Fälle handelt es sich jedoch nicht nur um einfach psychisch minderwertige Persönlichkeiten, sondern um Kranke, bei denen sich eine bestimmte, näher zu diagnostizierende Grundkrankheit nachweisen liess. Also der chronische Alkoholismus hier zunächst als Symptom einer bestimmten geistigen Erkrankung aufzufassen ist. Und in diesem Nachweis erblicke ich das wesentliche Ergebnis meiner Arbeit’ (S. 293).

He goes steadily through his eighty-nine individual cases of extreme alcoholism,<sup>1</sup> inquiring into the previous history of the individual, and the history of his family. He finds as foundations for chronic alcoholism (i) *Epilepsy* (34), (ii) *Melancholic mania* (27), (iii) *Dementia praecox* (14), (iv) *Other Psychoses and Psychopathies* (9), and a remainder of five unclear cases. In other words, in more than 38% of cases he gives epilepsy as a source of the chronic alcoholism. Stöcker gives strong reasons for the antecedence of the psychical defect, and he throws out the hint that in many cases where the drinking of a

<sup>1</sup> His definition of alcoholic excess (S. 10) coincides almost with that used by us, and it is a pleasure to find that from the medical side our definition can be also considered a valid and practical one.

patient's parents has been given as the hereditary moment in cases of epilepsy and melancholic mania, we are really concerned with parents affected in the same manner, and only alcoholic in a secondary degree. In the absence of these special psychical conditions—in the case of chronic alcoholists, whose fundamental disorder is pure 'chronic mania'—Stöcker finds that, in general, long years of marked drinking, including spirit drinking, do not in the least affect their mental capacity, power of work and energy; there is no question of imbecility.

'Im Gegentheil, bei dieser Art von chronischen Säufern ist man oft erstaunt über die erhebliche körperliche und geistige Rüstigkeit in hohem Alter' (S. 295).

Much the same view has been expressed by Chroters, who holds that the abuse of alcohol is a sign of degeneracy. Not opportunity to drink, not moral influences are the causative motives in alcoholism; the ground must have been prepared by the presence of degenerative factors.

It will, I think, be clear to my readers that it is not the statisticians of the Eugenics Laboratory alone whom Sir Victor Horsley—in the first place a surgeon—has to meet. There is a growing and really scientific medical school of alienists, whom he may much more usefully spend his time in opposing; and the only way in which he will be able to do this is to place observation against observation, and show in statistically significant numbers that sound stocks which take to drinking *de novo* will *de novo* produce defective children.

For the Eugenist the question is a vital one; he belongs to a body growing in numbers and weight in the state. Is he, admitting as he must do that alcoholism is a great national problem, to talk vaguely of alcohol as a racial poison and throw his influence into the existing temperance

movement? Or, shall he on the other hand attempt to study it for himself, with no initial bias? Here in a nutshell is the fundamental division between the two attitudes:

(i) All use of alcohol will lead *pro tanto* to defective children. Its abuse is due to opportunity and to defect of moral influence.

(ii) The abuse of alcohol is one of the stigmata of degeneracy. It is not the cause of degeneracy but its product. As the production of degeneracy—whether in the form of mental defect, epilepsy or insanity—is checked, to that extent the abuse of alcohol will be checked.

The acceptance of one attitude involves the demand for the cessation of all import, manufacture or sale of alcoholic drinks. The acceptance of the other demands the cessation of parentage on the part of the epileptic, the insane and the mentally defective.

It is for the Eugenist to consider the evidence for either policy, uninfluenced by rhetoric and by invective, and then to act in accordance with his decision. The two policies are not in my opinion compatible, for the evidence upon which (ii) is based shows that the practical sequence to the acceptance of (i), i. e. absolute prohibition, would not produce any permanent racial effect.

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